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ORIGINAL COMMUNICATIONS.

ARTICLE I.—*Letter from Paris.*

PARIS, Feb. 13th, 1854.

DR. JOHNSON,

DEAR SIR: There are so many objects of interest in Paris, connected with the Medical Profession, that one scarcely knows which to select for the subject of a single letter.

To the stranger who was familiar with the Institutions in 1840, it does not appear, on a return at present, that much progress has been made, either in the means of teaching the doctrines taught, or the qualifications of the Professors. The Museum of the School of Medicine, comprising healthy and comparative anatomy, specimens of materia medica, surgical instruments, etc., has been principally created since that period, and although not large for a national collection, is a precious addition to the means of instruction, and contains many preparations of the nervous and lymphatic systems of unequalled beauty. This collection is due to Orfila, whose name it bears. In addition to this, a new museum has been collected at the dissecting amphitheatre of Clamart, which contains some valuable pieces, but its situation, and the rules in regard to entrance, render it useless for the purposes of instruction. None of the hospitals have collections except that of Val-de-Grace, and most of the valuable specimens which might be derived from them, are lost for want of care. The Musee Dupuytran has received con-

siderable additions, but at the present time is inferior in every department except that of bones, which is very perfect.

The changes in the School of Medicine have not all been for the best. The death of Orfila left a void, which no attempt will be made to fill, the chair which he occupied being abolished. Dumas has also retired from teaching, and in the place of the two chairs, one of medical chemistry is to be instituted, and to be filled by Souberain, already distinguished at the School of Pharmacy. Trousseau, the unrivalled Professor of Materia Medica, has been transformed into a teacher at the Hotel Dieu, where he lectures to a small number of students, his lectures revolving around the orifice of the larynx, and scarcely ever embracing more than certain diseases of the air-passages. Andral is at present but little followed, and shows strongly the marks of advancing age, and the same may be said of Cruveilhier. On the other hand, Denonvilliers has succeeded Brechet in the chair of Anatomy, and gives a brilliant and popular course in place of that but little followed of his predecessor. Malgaigne has succeeded Blandin and his course is much admired and followed. In the Medical Clinique Bouillaud, Piorry and Rostan are still the Professors. Certain peculiarities of the first-named have so increased upon him, as to render his teaching intolerable. The second is still occupied in percussing the spleen, and marking its outlines, while Rostan continues his sound and valuable teaching in his wards at the Hotel Dieu. In the Surgical Clinique, the Professors are Roux, Velpeau, Laugier and Nelaton; the two former, too well known to need to be spoken of, but gradually declining from the effects of age; the two latter, highly popular, and rapidly advancing to the summit of reputation. The Clinique of Accouchments is directed by Paul Dubois, as formerly, than whose teaching nothing more admirable can be desired. It will be seen that in the Faculty, the gain scarcely compensates for the loss, and if the reputation of the School of Paris depended upon that alone, it might well be supposed to be upon the decline; but such is not the case, and the able corps of hospital physicians and surgeons, with the agreges chefs de clinique, internes, etc, keep up the public and private course to their former standard of excellence, although Louis has retired from his wards, and left vacant a place which cannot be filled.

Among the most notable defects of French teaching, may be noticed the neglect of general pathology and the use of the microscope. Robin is nearly the only teacher of the microscope, and his class is generally confined to a small number of American students. Nachet, one of the principal manufacturers of microscopes, told me that his best customers were from America, and that nearly all his most expensive instruments were sent there. The manufacturers of anatomical models of papier mache, plaster and wax, the preparers of skeletons and specimens of natural history, and the instrument makers, all assured me that their best customers are Americans or English. Under the head of Instruction, I should by no means forget to mention the course of Bernard, at the College of France, on Physiology; strangers, however, are apt to be disappointed in his course, as his brilliant discoveries lead them to expect too much. In other parts of his course, not relating to his own particular discoveries, he principally follows Magendie, whose place he supplies. I think that the premature publication of his opinions regarding a retrograde circulation from the veins into the arteries, through the kidneys, has tended to lessen the confidence of his hearers in his statements. His work on Experimental Physiology, which has been announced, will probably not be issued for a long time. At the side of Bernard, in the College of France, is Coste, whose lectures upon the development of organized bodies, is the most eloquent and instructive course I have ever listened to. The substance of his course is contained in his great work entitled "General and Special History of the Development of Organized Beings," which has reached its third number, being published by the government. He is known in America from his efforts in introducing the artificial breeding of fishes, a process now employed in all the different countries of Europe. I saw in his private room in the College of France, a number of troughs of earthenware, containing the eggs of fishes of the choicer kinds, such as salmon, from the lakes of Switzerland, and other kinds from the different rivers of Europe. Prof. Coste informed me that the eggs are transported without difficulty, a distance of 800 miles by land, and that they could easily be sent to America without injury. These eggs are of the

size of peas, of a bright red color, and to be hatched are placed upon little racks made of willow twigs, which are fixed in the troughs so as to be covered with water, and a gentle current is kept playing over them. I saw them in every different stage of development, from the simple egg to the fish just bursting from it. It is a curious sight, and seems to promise much benefit to man, by supplying fresh and wholesome food. It has, however, its difficulties. Like other orphans, a large portion of these fish brought up by hand, die before attaining maturity, and it is yet doubtful whether the plan will be successful, excepting with regard to those fishes found in the waters where the breeding is carried on; in this case there seems no reason to doubt its full success.

An interesting fact has incidentally been noticed by those engaged in observing this process of hatching; it is that a great number of monsters occur amongst the newly-hatched fishes, a number much larger in proportion than occurs in the mammalia.

In speaking of the scientific men of Paris, you will naturally expect me to say something of Lebert: he does not reside here at present but holds a professorship in a German University. His great work, however, on which he has been engaged for many years, is about to be put to press, and it is confidently expected that it will be by far the most perfect treatise on morbid anatomy which has ever been published. Bailliere, the great publisher, showed me some of the drawings which are in his hands to be engraved, and they are indeed magnificent. The work is so expensive that it was difficult to find a publisher to undertake it, but as the Minister of Public Instruction advances a certain sum towards the expense, and the Prussian government subscribes for a certain number of copies, Bailliere was induced to undertake it. The first number will be issued soon. While speaking of forthcoming works, I may mention that Guerin has one in hand and far advanced, on the subject of deformities, which is to be illustrated in the most perfect manner by beautifully-executed and colored plates. M. Guerin has devoted a great part of his life, and a considerable sum of money to this object, and has collected a museum comprising about 3000 pieces, illustrating every different point of the subject. This collection, which may be regarded as a national

one, is now offered for sale, and it were greatly to be desired that it should be transferred to some institution in America; as may be supposed, from the labor and money expended upon it, the price at which it is valued is not small. It may be interesting to some of your readers to know that M. Guerin, who was appointed by the Academy of Medicine some years since, to make a report on the subject of cholera, has nearly accomplished his task, and is about to submit it. After the fullest investigation and the examination of innumerable documents and publications of every kind, he assures me that he is fully convinced of its communicability, but only in certain conditions, and in a manner peculiar to itself. I think this opinion is beginning to have great influence here, since the cholera patients, which were at first carried into all the wards, have lately had particular halls assigned to them, separated as much as possible from the others. So long as they were carried into the general wards, the number of cases regularly increased, but so soon as the change was made, they diminished. As this is a doctrine which I had advocated for many years, it is gratifying to find its truth at length being recognized, more particularly as it is the only means by which the spread of the disease can be explained, and its ravages, to a certain extent, controlled. If time and space permitted, I should still have much to say of societies, journals, medical doctrines and men, past and present, but as I have occupied enough of both, they must be deferred to another time, or entirely omitted.

Very truly yours, B.

ARTICLE II.—*A Case of Caries of both Hip Joints—Uncertainties of Diagnosis—Memoranda of Treatment.* By A. Stone, M D.

But few men are endowed with that genius and that penetratin power of observation and combination, indispensably requisite for a revolutionary movement in science, and but few have inherited the lot of being placed in such an advantageous position of developing and applying those gifts of benevolent nature. At any rate, the writer hopes not to range under either category; he may, however, become the instrumentality for the advancement of

science, by serving as an exponent of the scientific inquiries, experiments and results, made and attained by others.

The polite invitation extended to me by Drs. Bauer and Barthelmess, physicians to the Orthopedic Institution of this city, has afforded the writer the rare opportunity of daily witnessing, for several months past, cases under treatment, developing some of the most instructive principles of pathology and cure pertaining to deformities and joint affections. From among the number of highly interes ing cases, the communication of the following, it is thought, may be very acceptable to the press on account of its instructive character in diagnosis, as well as the treatment adopted:

William Carter, aged 5 years and 8 months, had enjoyed excellent health up to April, 1852, when he was observed to assume a peculiar manner and gait in walking. According to the statement of his attending physician, Dr. Kalt, both hip joints were immovable, sore, and the legs firmly kept in abduction; in consequence of which the patient, when walking, was obliged to turn his body on the longitudinal axis of that leg which was then on the ground, thus succeeding in putting the other about six inches forward, and the same operation was then repeated by the first leg and so on. The symptoms increased, however, with such rapidity, that locomotion became altogether an impossibility.

The pain within the affected hip joints grew more intense, and the separation of the limbs wider. The opinion previously entertained by the attending physician of the case, was that the synovial membrane was affected by a scrofulous rheumatic inflammation, and the treatment pursued had been in accordance. Baths, superficial derivation, constitutional treatment and constant rest, alleviated the severity of the symptoms—the left leg, especially, approaching more towards the perpendicular of the body. The new position of the limbs, however, being totally fixed, afforded no advantage as far as locomotion was concerned. Other symptoms made their appearance in spite of the greatest care and attention from both physician and relatives. The affected joints became distended, and fluctuation apparent. The left leg was rotated outwardly and flexed both in hip and knee joints, the latter resting on and across the middle of the right thigh, while the right leg

remained almost straight, but at the same time being rotated outwardly. In this state the patient was received into the Orthopedic Institution.

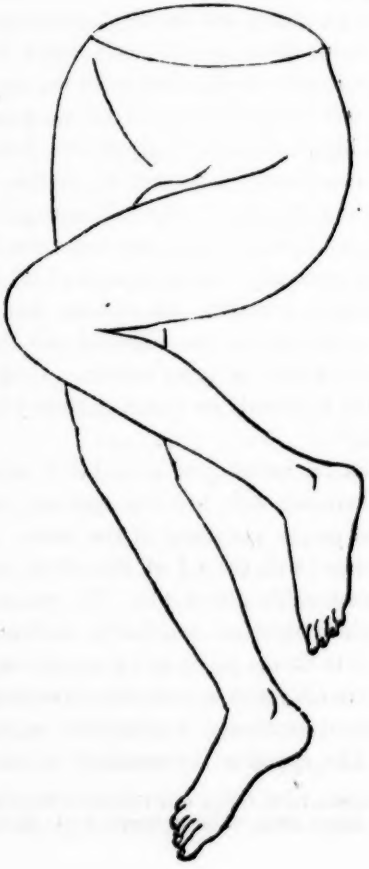
The case-book of this establishment contains the following notes on the case: Great pallor and emaciation; hectic fever, appetite good, digestion regular; dermoid surface dry and sluggish; sleep undisturbed; pain in the affected joints when moved, but not otherwise; general strumous appearance; induration and enlargement of lymphatic glands; discharge from one ear; thorax rather flat; auscultation and percussion discovered no abnormal sound; pelvis in a state of declivity, the left side lowered, the right elevated; the left leg consequently elongated, and the right shortened, in conformity with which the spine shows an inflexion towards the left, and the rima natum oblique towards the same side; the angle of the inclination of the pelvis normal, *therefore no anterior curvature of the spine;* both hip joints exceedingly painful, both as to pressure and attempted movement; indistinct fluctuation, no redness or heat, no visible sign of abscess; the left bent up in hip and knee joint, and in opposition to the right; the toes turned inwardly; the right leg rotated outwardly, nearly straight; both legs immovable within their respective sockets, all motions being made in the lumber portion of the spine; the adductor and flexor muscles of the right thigh in a state of great tension. Under the influence of chloroform, the legs could be moved in their joints, but no crepitus perceptible.*

Treatment: liberal quantity of animal food, wine, ale, brandy, cod-liver oil, alternated with iron and quinine; extension of the affected limb and proper placement of the same. This operation was easily performed with the aid of chloroform, and the patient felt perfectly comfortable afterwards. The patient was placed in his apparatus, which consisted in a double inclined plane, ingeniously constructed to fix the pelvis and limbs, at the same time allowing any passive alteration as to degree, extension, abduction, or adduction. Patient continued comfortable; appetite and sleep undisturbed. The apparent inconvenience of the new position,

* The affixed diagram, taken from a cast in plaster, illustrates the peculiar deformity which the patient presented on his reception into the Institution.

did not have the least unfavorable effect upon his general health, on the contrary he continued to improve rapidly considering his past enfeebled state of body.

The opening of the joints for the purpose of assisting nature more speedly to throw off the morbid matter and thereby reestablish a recuperative action was at this time, (the time of stretching the limb) a mooted question by the able surgeons of the institution, but postponed on account of the probability of the purulent matter becoming absorbed, after the parent cells having bursted and rearranged to other organisation capable of absorption, — an opinion



sustained by able pathologists — Professors Alonzo Clark, Paget, Miller and others — and indeed this hope, in this case at least, seemed to become realized, for the fluctuation of the joints became much less perceptible during the subsequent weeks of treatment; but then it increased again and so rapidly in the left hip joint that the matter could be felt quite distinctly. At this period of the case, an opinion was expressed by the surgeons of the institution, that a rupture of the synovial membrane had taken place, and a free opening of the abscess, and an enlargement of the spontaneous opening of the capsular ligament was rationally indicated. Consequently in presence of Drs. Sayre, Surgeon of Bellvue Hospital, N. Y., Kolt, Stoltz, myself and others, a large incision was made, the patient being under the influence of chloroform.

A large quantity of pus escaped. By digital examination, the great extent of the ulceration as well as the perforation of the joint was ascertained, being the posterior wall of the capsular ligament. A minute examination discovered the joint to be in the acetabulum, although the head of the joint was denuded and the connecting ligament destroyed, the acetabulum not being increased in size.

Injections of warm water was freely made, washing out both the cavity of the abscess and the joint, and finally filled with lint; the opening in the capsular ligament being large enough to allow the free discharge of the morbid contents of the joint. The patient having lost but a few drops of blood rallied very soon and expressed himself as being greatly relieved and comfortable. But a slight operation followed that operation both locally and constitutionally, healthy granulations and pus made their appearence and in six weeks the whole cavity was filled and consolidating, the patient at the same time improving generally.

Six weeks after the operation of opening the first or left hip joint, soreness and apparent inflammation was manifested in the right joint also: the patient was troubled with chills and occasional exacerbations of fever and dryness of skin and other constitutional symptoms, which together with the fixed condition of the joint warranted the unanimous conclusion in full consultation with the surgeons, called in for that purpose, that the cavity of the joint was filled with purulent matter and the whole integrity of the joint

seriously implicated. Although fluctuation could not be distinctly made out, the various considerations induced to a certain diagnosis by first exploring with a trocar. This was introduced superiorly and posteriorly of the great trochauter and under the joint at the posterior circumference, between the cotyloid cartilage and caput femoris. A column of thick pus immediately ascended the canular, which was now removed and an incision made at the same place about 6½ inches long, and the articular sack opened along the posterior hemisphere. About 6 to 9 ounces of consistent purulent matter escaped. The articular cavity, in as far as it could be ascertained, felt rough, both the greater part of the acetabulum and the posterior part of the femoral head being denuded; the ligamentum teres however being not destroyed. Caput femoris was found to be in its normal place, and the leg could not be rotated inwardly. I need hardly state, that crepitus was subsequently heard and felt. After the patient had been properly dressed and fixed in a leather splint, he revived and felt comfortable and relieved. The third day after the operation, the patient was doing well, cheerful, and to all appearance sustained the effects of the operation exceedingly well.

This is a history of a case, and a treatment no doubt involving questions of the greatest interest for the profession.

My private practice has not yet afforded me an opportunity of testing the new treatment, but the cases I have observed in the *Orthopedic Institution* in this city, tend to prove its soundness and efficacy. There are many surgeons who no doubt forcibly condemn the practice adopted by the surgeons of this institution, but they will fail of overruling the arguments and results in favor of freely opening ulcerated joints, while on the other hand, arguments principally consisting in the much dreaded entrance of air into the articular cavity is anything but conclusive.

There are certainly a good number of cases on record,—lately increased by 3 others observed by Dr. Judkins, (Ohio Medical and Surgical Journal, No. 3, vol. VI.) which terminated fatally after the free opening had been made, but it remains yet to be decided whether the operation or bad food, air, want of attention and other necessaries, or the long postponement, are not chargeable with the

health of the patient. Dr. Judkins himself refers to the preponderance of errors peculiar in the hospitals of Paris, frustrating but too often the results of many skilfully performed operations, and by the greatest masters of surgical art, and exactly the same we see in Bellvue Hospital. On the other hand, we hear of successful opening of the joints both from British and German Hospitals, especially of John Gay, Esq. Surgeon of the Royal Free Hospital, London. At all events the results of the old school in reference to the treatment of ulcerative diseases of joints, are of so inferior an order that the new mode has at least a right to be fairly tested. Competent men, unprejudiced by any established opinion, may then decide whether the treatment advocated and practiced by Dr. Bauer at the *Orthopedic Institution* in ulcerations of joints in general, and especially of the hip joint, is correct and reliable or not.

Brooklyn, N. Y. March 4, 1854.

SELECTIONS.

From the London Lancet.

Recovery after taking a large dose of Hydrocyanic Acid. By W. M. BURMAN, Esq., M.R.C.S.E., Wath-upon-Dearne.

The occurrence I am about to relate took place on August 6th, 1853, at six P. M.; but before going into the detail I must premise that my father and myself are practising together, and use the same surgery. We keep our Scheele's hydrocyanic acid for dispensing much diluted,—namely, in the proportion of one minim of the acid to a fluid drachm of water; this is kept in four-ounce purple glass bottle. It so happened that a short time previously we had received a fresh supply of Scheele's acid in a bottle precisely similar to the one in which we keep our *very dilute* acid. This strong acid was put into a cupboard under lock and key, but on the day in question had been taken out to replenish the dispensing bottle, and then left on the surgery counter. Soon afterwards my father returned from his afternoon ride, and being troubled with slight dyspepsia, went into the surgery, as was his habit, and mixed himself a draught containing a little aromatic spirit of ammonia and bicarbonate of soda in two ounces of water, adding to it a fluid drachm of the *very dilute* acid, *as he thought*, but he took it out of the bottle of Scheele's acid standing on the counter. He drank this off, noticing nothing peculiar in the taste. In a few seconds afterwards, upon looking at the bottle, he thought the label appeared cleaner than usual, and the idea flashed on his mind that perhaps he had taken the strong acid. A glance at the proper dispensing bottle, which was in its place, confirmed this. He directly poured half an ounce of aromatic spirit of ammonia into the measure, and drank it, *he believes* without water, but he may have added some. He then walked into the house to the bottom of the stairs (a distance of about sixteen yards), and called me; I came down stairs directly and went into the surgery, where I found him standing at the counter with a glass measure in his hand. He said. "I have taken a drachm of the acid; what can be done?" adding, "I have since taken half an ounce of aromatic spirit of ammonia." There was nothing in his appearence to attract my attention, but he spoke hurriedly, expiring deeply at the same time; he then sat down on a chair. I immediately put some crystals of sulphate of iron into a measure, adding one drachm of the tincture of the sesquichloride of

iron, and one ounce of water, stirring the mixture vigorously, to get as many of the crystals dissolved as possible in the time, and gave it to him to drink, which he did. I had no liquor potassium at hand, or I should have added some to the mixture before giving it to him, to precipitate the mixed oxides of iron; as it was, I was obliged to trust to the ammonia he had taken. My object in giving him this was to endeavour to get the mixed oxides of iron in contact with a least part of the acid in the stomach, and thus to convert it into Prussian blue, as suggested by Messrs. Smith of Edinburgh. Directly after drinking this, he began to breathe deeply, pressed his hand over his forehead and top of his head, and said: "Oh, I feel very queer; had I not better go out of doors?" I said "Yes" and with my assistance he staggered out, and dropped senseless in a sitting posture on a stone step at the surgery door. This was just about *two minutes* after taking the poisin, as ascertained afterwards by going through the same routine. From this time he recollects nothing till twenty minutes afterwards. His breathing was now slow and very deep; his eyes turned up completely under the upper lid; pulse moderate in volume, but very slow and intermitting. I hastily took off his neckerchief, fetched a large pitcher of water, and poured it over the back of his head and down his back. This produced no apparent effect at the instant, but a few seconds afterwards he vomited a mouthful of glairy mucus. This was nearly four minutes from the time of taking the poison. I sent for another pitcher of water, and during the interval again examined the pulse, and found it slower than before, feeble, and threatening to stop altogether; respiration very slow and deep, irregular, with blowing expiration and puffing of the cheeks; a little frothy mucus ran from his mouth. At this period the relaxation of the whole body was as great as that of a person recently dead. I now gave him another dose of the iron mixture as before, adding a little aromatic spirit of ammonia. By speaking loudly, shaking him, and pouring it into his mouth, I got him to swallow the whole of it.

Meantime the servant returned with another pitcher of water (about four pints), which I instantly poured down bis back (five minutes after taking the poison). This appeared to rouse him a little, and directly afterwards he vomited about two ounces of a deep bluish-green liquid; this I was glad to see, for I hoped that partial decomposition of the acid had taken place. The pulse and respiration were slightly quicker and more regular; pupils still quite invisible; conjunctiva injected; face livid. After an inspiration more than usually deep, he opened his mouth wide, and stretched out his arms; a spasmodic flutter passed over the face, and then over all the body, and I expected fully to hear a scream,

as I had generally noticed in animals, or that he was about to have a convulsion; but just as it begin to pass off, he vomited twice, and seemed relieved by it. The vomited matter was still bluish-green, and this time mixed with pieces of half-digested meat, which had been taken at dinner, four hours previously. I next gave him two drachms of aromatic spirit of ammonia in one ounce of water, and with some difficulty got him to swallow it. Having again filled the pitcher I poured the water down his back as before, and directly afterwards he vomited two or three times (seven minutes from the time of taking poison). The respiration and pulse were now very much better, the latter being quicker and pretty regular. Soon after the vomiting, I noticed a little movement of the hands, apparently voluntary. I now hoped the immediate danger was passed (eight minutes), so I watched the symptoms for a minute or two, and then the pulse beginning to falter a little. I gave him two drachms of aromatic spirit of ammonia in one ounce of water, which, with a little difficulty, he swallowed. I tried to get him to speak, or to give any evidence that he understood what was said to him, but failed to elicit any sign of consciousness from him; still I considered him tolerably safe, for the pulse was pretty strong and regular; the countenance was also now regaining its natural appearance, and the extreme flaccidity of the limbs was nearly gone. After about five minntes more had elapsed (fifteen minutes), he opened his eyes, and gave utterance to an ejaculation, and evidently understood, to some extent, what was said to him. Soon afterwards he complained of his trousers being wet, and this is the first circumstance that he remembers after leaving the surgery more than twenty minutes before. We presently got him into the house, and gave him a little hot brandy-and-water. I was now anxious to get him to bed as quickly as possible, judging that rest in the recumbent position would most conduce to his recovery. After sitting ten or fifteen minutes, he was able with great difficulty and with our assistance to walk up-stairs, but vomited several times during the transit. We got him into bed, and soon afterwards, turning to me, he said, "You should have used the cold affusion," and seemed quite surprised, when I told him that I had done so pretty freely. During the evening, he took a simple effervescent draught now and then, and presently he dropped asleep. He passed a good night, and next morning complained of pain across the loins, but was otherwise pretty well, except a feeling of general weakness. I ought to have mentioned that my father is about sixty years of age, and of a strong constitution.

A day or two afterwards I analysed the acid. I measured one drachm in the same two-ounce measure that my father used, and put it into a minim measure; it filled up to seventy minims. I diluted this and then added a solution of nitrate of silver till no more

precipitate was produced. I repeatedly washed this precipitate, and dried it carefully for a long time; but to make sure that it contained no more water, I put it into a warm oven for an hour, during which time it lost nearly one-fifth of a grain It now weighed exactly twelve grains, which would be equal to 2.4 grains of anhydrous hydrocyanic acid, *the quantity my father took.* As 100 grains of the acid filled up to 105 minims in the same small measure, this would give 3.3 as the per centage of the anhydrous acid in the Scheele's acid used.

I have been thus minute, perhaps unnecessarily so, in the detail of this case, because it presents many points of unusual interest; some of these it may be well to recapitulate very briefly: —

1stly. This is the largest quantity recorded (so far as I know) after taking which recovery has ensued.

2ndly. The quantity of acid taken was *measured*, being certainly no less than one drachm of Scheele's strength (at 3.3 per cent), and being equal to 2 4 grains of real acid.

3rdly. The time at which insensibility supervened after taking the poison is accurately known, namely *two minutes.*

Lastly. The good effects of the cold affusion and the probable decomposition of part of the poison by the mixed oxides of iron.

The efficiency of the Sulphuric Acid in Diarrhœa. By GOODEVE BOWRA, ESQ., M. R. C. S. E.

As much has been said and written lately on the treatment of diarrhœa, I beg to offer my testimony on the happy effects of sulphuric acid on persons of all ages, with the full conviction that it is the quickest and most palatable, consequently the best remedy for that disease.

I was requested two years ago to send medicine to a young lady subject to diarrhœa, who had taken a passage to China in a ship not carrying a surgeon. Thinking there must be some mistake, I went on board the vessel and saw the chief officer, who thought himself quite competent to treat (from books and a medicine chest) any disorder likely to arise on the voyage. On asking this gentleman what he would do in cases of diarrhœa, his off-hand reply was, "Never care for that; always carry plenty of sulphuric acid on board, and I have never known it fail."

On the same evening I was called to a lady, seventy-five years of age, very subject to these attacks, which always lasted a week or more, and as it was of great consequence she should return into the country in two days, I made up my mind to try the sulphuric acid, believing I should not be more successful with the old chalk mixture than her medical friend at home.

The following morning I found her up, and so much better, that

she had only taken two doses; I gave her a third; which completely cured her. Since then I have used nothing else (save a mustard poultice) in all cases, either with or without pain, both in private practice, and at an institution to which I was attached, and I confidently state that during the whole period I have used it I have not met with an unsuccessful case. The only difficulty is, to persuade some people that acids would not increase their disorder, particularly those accustomed to the old chalk and aromatic confection treatment.

I now constantly recommend patients subject to diarrhœa, or who are nervous about cholera, to keep a bottle of the sulphuric acid mixture in the house, and on the first symptom of their complaint instantly to take a dose, which is generally sufficient to effect a cure. Many of these patients (I fear, jokingly) tell me they shall expect double charges, as two or three draughts of the acid mixture have more effect than the same number of bottles of the chalk mixture; and I may add, I feel so satisfied with the success of a two years' trial, that I have no hesitation in asserting it as my full belief, that deaths from cholera and diarrhœa would be very materially diminished if the authorities would appoint an agent in all poor neighbourhoods, to give a dose of the sulphuric acid mixture to every necessitous applicant suffering from bowel complaint. They would have plenty of persons desirous of availing themselves of this remedy, if I may judge from the gallons I gave away last summer.

Charterhouse-square, 1854.

P. S.—It may be as well to state that the cost of the above remedy is one shilling a gallon, or thirteen doses a penny. If administered as on board the vessel to which I have referred—that is to say, without any colouring matter, the six gallons of mixture will cost one penny, so that the expense to be incurred by the government in relieving the suffering poor would be but trivial.

From the London Lancet.

Medical Society of London.—A Paper on the Excretions as Guides to the Administration of Remedies in Rheumatism and Rheumatic Gout. By Dr. Fuller.

The author began by stating that no great advance can take place in our knowledge of disease, nor any material improvement in its treatment, unless we endeavour to discover the primary cause of each morbid action, and trace its influence in modifying and deranging the various functions of life. After briefly illustrating this important truth, he proceeded to point out how close a relationship the amount and character of the various excretions must

necessarily bear to the condition of the general system, and how certain an index they afford to the energy of those processes by which the effete materials of the body are got rid of. Hence he deduced the inference, that no plan of treatment can be proposed, with a well-founded rational prospect of success, which is not based on a due regard to the different excretions, and varied with their varying condition.

He then proceeded to apply this general law to the elucidation of the treatment of rheumatism and rheumatic gout, and showed that, inasmuch as these disorders depend on the presence of a morbid matter, the product of imperfect or faulty assimilation, a proper action of the excretory organs is more than usually necessary. The alterations usually produced on the character of the excretions by the existence of rheumatism and rheumatic gout, were next alluded to, and some remarkable exceptions pointed out; and the author stated his opinion that the chief aim of treatment should be, by producing, as far as possible, an increase of those excretions which are scanty or deficient, to make each and all of the excretory organs assist in eliminating the *materies morbi*, and to endeavour, by close attention to the character of the excretions, to correct their morbid condition. He then referred to the good effects resulting from treatment regulated according to these views, and mentioned many facts to prove and illustrate the ill success which attends every mode of treatment in which the condition of the excretory organs is not attended to. Having fully established these general principles, his next endeavour was to point out the means by which they can best be carried out. He first premised that if all the excretions are scanty or suppressd, and if at the same time the pulse be full and bounding, venesection will not only relieve the general tension of the system, and alleviate the pain and general distress, but will be followed by action of the excretory organs. He then proceeded to discuss each of the excretions separately, and in regard to the perspiration, stated his conviction that much mischief is often done by interfering with Nature's mode of operation. No bath should be administered as long as perspiration takes place naturally, but if the skin is dry or acting sluggishly, a bath is essential to stimulate its action, He strongly recommended a water bath of 100° Fahr., rendered alkaline by potash or soda, but in the event of its being impracticable to make use of a water bath, the vapour or hot-air bath may be substituted. In either case the effects of the bath should be sustained by guaiacum and Dover's powder, or tartarized antimony and saline diaphoreti medicines. The only exceptions to this general rule are met with in persons of a weakly constitution, or towards the close of lingering cases. In Such in-

stances the perspiration is sometimes very profuse, but loses its distinctive empyreumatic odour, and much of its peculiar acid character, and is accompanied by a soddened state of skin, a quick, feeble, irritable pulse, and not unfrequently by an eruption of sudamina. Tonics, such as quina and sulphuric acid, are then requisite, instead of diaphoretics and salines, and as soon as all feverishness has subsided, the caution administration of iron is almost always beneficial. The urine was next appealed to, and made to furnish its quota of evidence. Dr. Fuller insisted strongly on the fact that the mere appearance of the urine, its colour, clearness, or torpidity, affords no clue to its real condition—to the amount and character of its solid ingredients, which can only be ascertained by careful examination. This he proved by reference to facts, and then went on to show that the amount to solid matter excreted by the kidneys is usually much diminished, and that diuretics are necessary to increase their action. A most important question is, as to what diuretics should be employed. A state of congestion and irritation exists consequent on the abnormal condition of the blood, and the exhibition of ordinary diuretic medicines, which operate merely as renal stimulants, is more likely to increase that congestion, than to cause an abundant flow of urine. Hence cantharides, squills, nitric ether, scoparium, and other similar remedies are of little or no service, whilst alkalies and the neutral salts, such as the acetate of potash and the potassiotartrate of soda, which correct the condition of the blood, are most active in promoting diuresis. So also are the preparations of colchicum. Water too proves of service by promoting the absorption of the salts, and assisting not only in the excretion of the solid matters, but in their subsequent solution, The condition of the urine, as to the dose in which alkalies should be administered, the frequency of their repetition, and the propriety of persevering in their use. The alvine evacuations were next referred to, the necessity for strict attention to their character was pointed, out and the peculiar conditions which call for the administration of different remedies were clearly indicated. Dr. Fuller insisted upon the powerful cholagogue influence of aloes and the acetous extract of colchicum in these cases, and urged the administration of these remedies, in conjunction with blue pill or calomel, whenever it appears desirable to excite an increased flow of bile. The principles of treatment already laid down were next applied to chronic rheumatism, and subsequently to rheumatic gout, and it was shown that in the latter form of disease the treatment requisite to produce the desired effects need considerable modification according to the stage of the disorder, and the constitution of the patient. A disregard of this fact, together with the practice, too prevalent in the present

day, of prescribing each medicine separately, constitute, in Dr. Fuller's opinion, the chief cause of the frequent failure of the treatment ordinarily employed in rheumatism and rheumatic gout, and form additional grounds for a close examination of the excreta, inasmuch as an examination proves that no two cases are alike, but necessarily require remedies differing widely in their character, no less than in the dose in which, and the period of the attack at which they should be administered.

Dr. Semple, after speaking of the unsatisfactory results of the treatment of rheumatism which he had formerly pursued, by bleeding, &c., observed that for some time past he had treated all cases of acute rheumatism with lemon-juice, and the result had been invariably satisfactory. For the first few days of treatment he placed the patient on strictly low diet, and administered the juice of six lemons daily; this, with opium, given in the form of the soap-pill, formed his entire treatment. The opium was given in doses sufficient to relieve the pain, and might consist of one, two, or even three grains. Under this plan the pain gradually became less, the fever subsided, and the disease abated. The return to health was more rapid than when exhausting treatment had been resorted to. Care was requisite, after convalescence, that the diet was not too stimulating. This treatment had the advantage of allowing us to use more energetic measures when any local complicatian, as heart disease, took place, as the system had not been previously exhausted by treatment. The plan had also the advantage of simplicity, and surely that was a very great one. He did not profess to determine the *modus operandi* of the lemon-juice, but its beneficial results were unmistakable.

Dr. Lankester, said that the difference of opinion regarding the treatment of rheumatism could only be satisfactorily terminated by the numerical method on an extensive scale. Were we to treat to disease on an empirical or a rational basis? He thought the Society was much indebted to Dr. Fuller for assisting us in arriving at a more rational mode of treatment. The treatment by lemon-juice was purely empirical, and no theory which had been advanced to explain its action was satisfactory. The plan proposed in the paper, as founded on the excretions, was a step in the right direction. With respect to low diet in the cases of rheumatism he thought that this plan must be resorted to with caution, as occasionally patients died from want of nutrition, particularly in cases in which the heart was diseased.]

Dr. Webster considered in the treatment of rheumatism the first point to decide was, whether it was inflammatory or asthenic. In the first of these forms low diet, perhaps bleeding, diuretics, diaphoretics, &c., would be indicated; but an opposite plan must be

followed when the disease was in a broken-up constitution. Lemon-juice he thought was often beneficial, but sometimes of no service. When combined with opium, he thought, in some cases at least, the opiate was the more powerful remedy.

Dr. Theophilus Thompson thought that in Dr. Fuller's paper the importance of the state of the excretions as an indication of the mode of treatment to be employed, had been overrated. The author had accidentally touched upon a more important point—viz., the state of the blood in rheumatism. In this disease we must have a careful regard to the general condition of the patient, as well as paying a strict attention to the state of the secretions, for a similar state of the excretions might exist under very different conditions of the system, and different modes of treatment be therefore indicated. The excretions, too, instead of being guides to treatment, might only show that the disease was passing off. Respecting lemon-juice in rheumatism, he had found it of more service in inflammatory cases of the disease, in which the patient was not robust, and depletion could not be resorted to.

Dr. Fuller, in reply, said that the object of his paper was not to embrace the entire subject of rheumatism, but to explain how far the state of the excretions might serve us as guides to the treatment of the disease. He deprecated the employment of lemon-juice, and thought that when it did do good, whether in rheumatism or gout, it was in persons who lived almost entirely on animal diet, and without vegetables.

From the New Orleans Med. and Surg. Journal.

Letters on Yellow Fever at Memphis, Tenn., in 1853. By Geo. A. Smith, M. D., Physician to the Memphis Hospital, and W. J. Tuck, M. D., of Memphis.

(We commend the following letters to the special attention of our readers. *Ed.*)

Memphis, Tenn., Dec., 1853.

Editor N. O. Med. and Surgical Journal.

Dear Sir—With the belief that it may be a matter of some interest to trace the phenomena of Yellow Fever when carried away from the head quarters of its epidemic fury, the following remarks have been written upon some cases which were brought for treatment to the Memphis Hospital.

The city of Memphis is well known to be by river about 800 miles above New Orleans, and is the lowest point of any considerable population which escaped the fearful epidemic of 1853, having been not only free from Yellow fever, but favored with a much healthier summer and autumn than has fallen to its lot for several

years. The mortality from May to August, inclusive, (generally the unhealthy season here) was in 1853 considerably less than half what it had been two preceding years.

The nearest point to Memphis at which Yellow Fever prevailed as an epidemic was Napoleon, Arkansas, about 200 miles below; and it ascended to no point above the city; so that Memphis was the lowest point on the Mississippi river, of any considerable population, which enjoyed this happy immunity. On the other hand, it prevailed with virulence at Natchez, Vicksburg, and several of the smaller towns intermediate.

All the cases which were brought to the Memphis Hospital, in number 62, were landed from the river, and contracted the disease in New Orleans and its immediate vicinity. The mortality, as might be expected was very great, from the fact that they were not landed till the disease had taken full possession of the system, and then carried the distance of a mile from the wharf-boat to the Hospital, on an open dray. Of the 36 who died, 17 lived less than twelve hours after their arrival.

Three methods of treatment were adopted, of which the most successful, in fact the only successful one, was the following: On their arrival, after a hot mustard foot-bath and being warmly covered in bed, a laxative, if necessary, was administered, of an infusion of renna, rhubarb and manna; cups and mustard to epigastrium, with ice given solid and iced mucilage; opiates, particularly the Black Drop, and milk punch ad libitum. Several cases were cured under this treatment, after the black vomit had been well established. The other methods tried were that of calomel and opium in broken doses, with twenty drops of laudanum—both entirely without success.

The question of the contagiousness of Yellow Fever was fully tested, and the result confirms the impression, that without the pre-requisite conditions of atmospheric and climatic derangement, Yellow Fever is no more liable to be communicated from one subject to another than gout or rhematism. At any rate the 62 patients who were treated in the Memphis Hospital were all of them distributed promiscuously through the wards of the institution; treated there throughout their progress to recovery or death, and no single instance occurred of the disease upon them.

The above brief statement is respectfully submitted to you. In relation to so terrible a scourge as the epidemic of 1853, it is presumed that as large an accumulation of facts as possible is most desirable—facts derived from observation made under all possible circumstances; and provided they are plainly and truthfully stated, ought to be acceptable, whatever the sources from which they are derived. The circumstances under which my observations were

made were in this respect peculiar, indeed almost unique, as Memphis was probably the only place where a considerable number of *imported cases* could have been treated, in a region itself exempt from epidemic influences.

Respectfully, GEO. A. SMITH.

REMARK.—We are at a loss to conceive how any one could expect benefit from doses of 20 grs. quinine and 20 drops laudanum in the advanced stages of Yellow Fever—say the third or fourth day. We have seen no such treatment recommended by any advocate of quinine, but on the contrary, always forbidden. Calomel and opium in this stage is not much better. F., *Ed.*

Editor N. O. Med. and Surgical Journal.

MEMPHIS, Tenn., March 19, 1853.

MY DEAR SIR—I applied a few days since to Dr. Smith, Physician of the Hospital here, in relation to the subject of your enquiry, and he informed me that he had recently transmitted all the statistical information connected with the subject to Dr. Hester, and which probably reached its destination about the time of his death. The Doctor wished me to refer you that letter. He informed me at the same time that there were 62 cases of Yellow Fever admitted into the Hospital, which were taken from the boats landing at this place and 36 deaths, and that in no instance was the disease communicated to others. As well as my memory serves me, we had only three cases of Yellow Fever in this city; two of these had been exposed to the epidemic in New Orleans, and probably contracted the seeds of the disease there; the third was a citizen of our place and had not been absent, and it was regarded at the time as a sporadic case, such as occasionally occurs in this city.

You request me to give my view in regard to the contagiousness of Yellow Fever. So far as I have been competent to investigate this subject, and so far as my own observation extends, I am a decided noncontagionist. During my residence in New Orleans, in the summer of 1841, and while one of the attending Physicians of the Charity Hospital during the epidemic of that summer, I think it was the unanimous sentiment of all the prominent physicians of the city that the disease was not contagious, nor do I think any one pretended at that time that the epidemic of that season was imported from abroad. Such, I believe, have been your own statements, as evinced in your writings on this subject. Whether the opinions of the physicians of your city have undergone any change in relation to this matter since the breaking out of the late epidemic, I have not been able to gather any accurate information.

Still the intimation has been thrown out, that circumstances have been developed in connexion with the recent epidemic, which have led some to alter their opinions and to favor their doctrine of contagion. However that may be, we are all perfectly well satisfied *here* that the disease could not be communicated by contagion in the atmosphere of our city; and it seems to me, if I understand the meaning of the word contagion, in its proper acceptation, that contagious diseases may be communicated and reproduced from one person to another, without any regard to the temperature or latitude; as for instance, small-pox, scarlet fever, measles, etc.

Now, let us look for a moment to the facts of the case in our city. We adopted no quarantine regulations; we had instituted no especial sanitary regulations with the view of preventing an incursion of the epidemic. Boats freighted with merchandize, saturated with the atmosphere of an infected city, landed at our wharf almost every day; a number of persons affected with the disease were carried through our streets to the Hospital, and some of them dying in private families, and not a case was communicated to any of the nurses, friends or physicians.

It certainly must be a very singular and unprecedented sort of contagious disease which could not be communicated in a single instance, under such favorable circumstances! when the streets of our city were as filthy as usual; when the weather was very warm; when there was no avoidance of exposure, and when every condition existed to promote contagiousness! Does any person suppose for a moment, that there could be brought into our city such a number of cases of any of those diseases ordinarily termed contagious, without the occurrence of a single instance of communication to the unprotected and exposed? During the month of September it fell to my lot to be called upon professionally to attend some cases of Yellow Fever occurring on the steamer H. D. Bacon, from New Orleans. When the boat arrived here some ten cases were taken off and sent to our Hospital; I was retained professionally to attend some of the officers to their point of destination, St. Louis. I must confess that I had some apprehension of danger when I first went on board of the boat; not from fear of contagion, but from the infected atmosphere, which I supposed might still have remained to some extent. The boat was a large one, heavily freighted with merchandize, and a large number of the crew and passengers were sick, some of whom died before reaching here. Here, certainly, was as fine and fair an opportunity for an unacclimated and unprotected person to contract a contagious disease as we can well suppose to exist; and yet what are the facts of the case? Not a passenger, out of some dozen or more, who took passage on the boat, above the infected region, suffered from any symp-

tom of the disease. And yet the second clerk, and the captain, who was on duty until he reached St. Louis, had contracted the seeds of the disease in New Orleans, and after I returned home to Memphis I learned that both had died of Yellow Fever several days after their arrival at St. Louis. I am happy to state that the several cases which were under my care were convalescent when I reached St. Louis, and I returned immediately home; and although exposed constantly day and night to whatever source of contagion there might have been, yet my health remained not only unaffected, but was better than usual.

It is said that the epidemic which has swept with so much fatality over the fairest regions of our southern country, differs in some respects from the preceding epidemics. We are disposed to believe the difference exists rather in degree than in kind. The cases we have seen here, imported from your city, have been just the same in symptoms as those we witnessed in New Orleans in 1841. It is true, the recent epidemic has been much more malignant and fatal than any of the previous ones; and it is stated that many more of those supposed to be acclimated were affected with the disease than at any previous period; but the main peculiarliy that has been insisted upon is, that the epidemic extended over a much larger scope of country than it has been previously known to do, and occurred in many towns and villages where it was never known before to exist; thus appearing to favor the opinion entertained by some, that the disease was transmitted to those localities by contagious influence, and not originating from local causes.

Now it is a very important question to determine, whether, in those cases, the disease did *originate* in certain localities, or was transmitted by persons affected with the disease, steamboats, etc.

We have long since learned enough of luman nature to know that the citizens of any particular town or locality are very slow to acknowledge their location a sickly or unhealthy one; and this is very natural; and no doubt many of the citizens of various villages in Louisiana and Mssissippi are disposed to attribute the visitation of the epidemic among them to importation from New-Orleans. Natchez has been in the habit of doing this for the last thirty years; and yet with all her quarantine regulations, as likewise those of Vicksburg, the disease has prevailed in those places with as much or more fatality than in New Orleans. It is true the quarantine regulations were not as rigid as they might have been; yet if we are correctly informed no steamboats from New Orleans were permitted to land, nor were any of the sick on those boats taken care of until the epidemic made its appearance in those towns. Now compare this condition of, things with that which existed in Memphis, not much more than a day's travel on

a fast boat. Here we imposed no restrictions—had no quarantine. As far mentioned, large quantities of goods, which had been remaining for weeks in New Orleans, were daily put off at our wharf and distributed through the city; many of these goods of a molient character, well calculated to act as fomites to convey contagious effluvia; and moreover, a number of persons affected with Yellow Fever, daily taken from the boats and dying within the precincts of the city, and yet not a single instance of its communication occurring.

But the enquiry is raised, if the disease was not communicated or transmitted from one point to another, in the region South of us, why did it occur and how did it originate in some of those localities hitherto regarded as healthy and where the epidemic was never before known to exist? We reply to this by asking, how did it originate in New Orleans, Mobile and other places, at the time preceding which the epidemic was never known to occur in those latitudes? The fact is, a combination of *local causes*, we believe, sprang up at that time; which combination had never existed previously.

We are very much inclined to the opinion, that there existed in the whole region of country embraced within a certain line of latitude South of us, a *certain predisposing epidemic constitution of atmosphere*, which aided in giving rise to so extensive an epidemic. We believe that in the same way there is a predisposing and peculiar constitution of atmosphere prevailing when cholera appears in our country, only the letter is more extensive, and is but little influenced by climate, whereas the former is confined almost exclusively to hot latitudes. The probability is that the increased fatality and malignity of the epidemic this year was the result of a greater degree of this predisposing influence in the atmosphere constitution. This being the case, and other causes, such as animal or vegetable miasm, being the same as in previous epidemics, we may readily explain why the recent one should have been more fatal and malignant as well as extensive in its scope. Now supposing this predisposing atmospheric constitution to exist throughout the whole of a certain region of a warm climate, may we not thus account for the development of the disease in certain localities where it has never occurred before, without resorting to the doctrine of communication or contagion?

This opinion would seem to be corroborated by the fact, that in some of these localities it has been found impossible, from the best information we can obtain, to trace out the least communication of any kind with those towns or places where the epidemic was prevailing. And may we not in this way account for the exemption of our city and that of all other towns on the river north of a

certain latitude, say that of Napoleon or Lake Providence? In this city surely we had enough of animal and vegetable filth to produce disease, as well as every circumstanee to favor contagion, but we did not have the predisposing constitution of atmosphore. We are therefore disposed to believe that the disease is of local origin and not contagious; and we offer the above theory as a sufficient one, we believe, to account for the extension and prevalence of the disease in Southern localities not hitherto subject to it, without the necessity of resorting to the doctrine of contagion, importation or transmission.

Yours, respectfully,

W. J. Tuck.

The Treatment of Erysipelas Analyzed. By Sanford B. Hunt, M. D.

I propose to bring the various remedies employed in the treatment of erysipelas to the test of a critical analysis, based on the ascertained facts of the disease; laying down first the proposition, that we should exhibit no remedies without knowing why we do so.

When a few years since malignant erysipelas first prevailed to an alarming extent in this section of the country, the views of medical men, both as to its pathology and tendencies, and as to its treatment, were unsettled and unsatisfactory. The epidemic of 1844 and '5 was exceedingly fatal, as might have been expected in this state of medical opinion. Sundry points of pathology were at that time settled in the minds of thinking and observing men.—Among these the following may be laid down as propositions, then verified and placed among the facts of the profession:

1st. Erysipelas is a contagious exanthem, originating in the presence of a specific blood poison, either conveyed into the system by contagion, or developed there by certain morbid processes not understood.

2d. The tendency of erysipelas is toward the recovery by the self elimination of the blood poison.

3d. The action of this poison depresses the vital powers, but not usually to a fatal degree. When the poison is directed from the surface toward the nervous centres, we shall have symptoms much more alarming than when it expends itself in cutaneous inflammation.

Probably these propositions are no novelties, and will be readily acceded to by all who have carefully watched the disease in question. I shall not, therefore, enforce them by any agument, but shall confine the scope of this article to sundry deductions as to treatment, derived from these data.

1st. If erysipelas is a contagious exanthen, analogy would lead us to suppose that its management should be governed by the same rules which guide us in other principle exanthemata, viz., scarlatina, rubeola, variola, varicella, and continued fever. All these disorders belongs to the same family, and are subject to the same laws. All have their origin in contagion, though some of them may be self developed. All of them depend upon blood poison as their cause. All are self-limited; in all of them an abortive treatment is perhaps impossible; in all of them the degree of danger depends upon the degree to which the nervous centres are affected by the poison; and in all of them that treatment will be most successful which most favors the elimination of the specific poison. These analogies are worthy of consideration. We may argue with tolerable certainty from one to the other, and may readily conclude that one principle of treatment should govern the whole. And in the whole art of therapeutics no principle is better established than that depletion is inadmissible in all this class. Erysipelas is a blood poison. Therefore any merely local treatment must be generally insufficient. I say *generally*, because there are many cases in which this poison expends itself almost as locally as does vaccinia in its usual mild career. Of course, in such cases local treatment will be not only sufficient but superfluous. Professor Bennett saw in the Hotel Dieu a number of cases which were receiving no treatment—M. Louis asserting that erysipelas of the scalp was never fatal. And this accords with all our preconceived notions of eruptive diseases. When the eruption comes out fair, thus affording the best chance for elimination;—when it occupies only its natural locality, the skin, and is thus uncomplicated with visceral inflammation, we expect a recovery *tuto, citoque, jucunde*, without much, if any medical interference, and the only judicious treatment in any of these diseases is that which directs the morbid matter in common with the whole tide of circulation toward the surface.

The elimination of the specific poison of erysipelas is conducted by the usual emunctories—the skin and the kidneys. Free diaphoresis exerts a most favorable influence upon the progress of the disease, tending to shorten the process of elimination. The occurrence of desquamation, even when no bullæ exist, indicates the effusion of fluid beneath the cuticle, and it is not improbable that this effusion substracts its quantum from the sum total of disease. The relative quantity of urea in the blood seems to have in this, as in all other exanthemata, an influence upon the severity of the disease. But what this influence is we do not know, though we may hope that with our present means of investigation the thing may soon become clear.

In erysipelas, as in other desquamative diseases, we find frequently—not always—the accompanying sign of albuminuria. This occurs at the period of declination, and depends upon the solution of the renal epithelium in the urine. That this is a part of the process of elimination is not proven, and it may be found to depend upon a local disorder of the kidneys, incident to the increased secretion of urea occurring at the period of declination. The plain indications of treatment derived from these natural phenomena, are to encourage the secretions of both the skin and the kidneys.

2d. The tendencies of erysipelas is toward recovery. This is proven by the small average of deaths—the recovery of very many cases in which the disease has run its course uninfluenced by medication, and finally by the analogy derived from the natural history of other eruptive diseases.

3d. Whenever the symptoms become alarming it is not from the acute or destructive character of the inflammation, but from a depression of the nervous energy. Gangrene is of extremely rare occurrence. Abscess is more common, but still so unusual as not to be classed among the natural sequelæ of the disease. When erysipelas assumes a bad type, we find the inflamed surface looses its lively red, and acquires a dark purple hue; the skin is dry; the urine suppresssed; the tongue has a dry brown crust; the teeth are covered with sordes; the pulse grows frequent and fluttering; delirium and coma are present; in a word, we have typhoid symptoms. All this retinue of bad signs have their origin in a morbid impression on the great nervous centres, and we readily draw the indication of a supporting treatment. If the disease pursues its natural course, finding outlet by the skin and kidneys, we shall have the vital powers unimpaired, the reason clear, and the pulse—the great indicator of nervous disorder—undisturbed. But when from any cause, the poison is retained in the circulation, and goes on increasing by zymosis, we shall find that the brain becomes congested—that the disease has left its wonted channels, and is expending itself on organs more important—upon the great moving power—the *nodus vitiæ* itself.

It seems to me that if what I have advanced be true, we have a sufficient basis for a scientific and successful treatment of erysipelas, which by a parity of reasoning will apply to the other eruptive zymotics. It is not probable that those things yet unexplained will have the same important bearing upon treatment as the facts already in our possession.

Treatment.—Most of our systematic authors speak of bleeding as admissible, and even praiseworthy, in the country, but not admissible in the form of the disease seen in the cities. But if what has been said about the tendencies of the disease to typhoid action,

the little danger of an unfavorable result from the extent or violence of inflammatory action, and the importance of maintaining the nervous system in full vigor, be of any moment, then is a bleeding in erysipelas as unphilosophical as in continued fever, variola, or any other exanthem. The safety of the patient lies in the activity and lively character of the local inflammation: for the extent of the inflamed surface is only an indication of the amount of blood poison and the activity of zymosis.

The diaphoretic treatment seems, at first view, to be theoretically and practically correct, as indeed it is. But to secure diaphoresis, we should not administer those drugs which, by their depressing influences, lessen the nervous energies—*e. g.*, antimony. There are, however, diaphoretics of a different class, to which no objections can apply, as the acetate and carbonate of amonia.—Whisky punch, which at first view would seem a good stimulant diaphoretic, is objectionable on account of the reaction and torpor of the nervous system following its use.

Diuretics are also indicated, but the antagonism of the skin and kidneys renders it difficult to meet both these indications at once. Perhaps colchicum, tending directly to the removal of urea, might be found the best of diuretics.

But even these indications, important as they are, yield to the necessity, in serious cases, of supporting the nervous powers.—Quinine is thus valuable, and here do we find what I conceive to be the true theory of Hamilton Bell's treatment by the Tinct. Ferri Muriatis. He himself argues that the capillaries are in an atonic state, and that the system being rapidly saturated by this powerful astringent, the atony is removed. Evidently if this were true, cold affusions would be equally successful. In the only case in which I have had an opportunity to observe the action of this remedy, it was commenced twelve hours after the appearance of the eruption. The pulse was 100, full, and firm. Fifteen drops of the muriated tinct. were given every two hours. A cathartic was given with the first dose. The patient was a blacksmith, aged 35. There was no other treatment, either general or local. Under the remedy he sweat profusely, and forty-eight hours after the treatment commenced, the pulse was 72 and soft, and the swelling, which had occupied the whole face above the mouth, closing both eyes, was declining. He convalesced rapidly. This is a single case. Perhaps a collection of cases might give a different result, though this case is similar to those reported by Dr. Bell in all particulars. In attempting to explain the action of the remedy we may say this much: The iron is a good tonic, and is *prima facie* as well adapted to the disease as quinine. Perhaps by the action

of iron the nervous system is exalted to a pitch which enables it to exert its whole energies in throwing off the poison.

Cathartics.—Probably there is no case of erysipelas which will not at some time in its course be the better for cathartic action.

Another corollary to our proposition is, that all local applications further than those tending to relieve itching and pain, are unnecessary and mischievous. If by local applications you cut short the inflammation, you lengthen correspondingly the disease. This may not hold true in a far advanced stage of the disease.

It will be seen that by this process of analysis from established facts, we rather subtract from, than add to our armamentaria against this disease. The whole treatment is reduced to a simple combination of tonics, diaphoretics, and diuretics. The peculiar remedies which have my preference I have indicated, viz., iron. colchicum, and the acetate of ammonia; but it is probable that other articles of the materia medica may fulfiill the same indications, nearly, if not equally as well.—*Buffalo Med. Journal.*

(Translated from the "Revue de Therapeutic Medico Chirurgicale.")

Therapeudics. The Wonderful Effects of Belladonna in the Third Stage of Pneunomia.

Case—On the 2d Oct. 1852, towards noon, M. X., a monk at Berguac, 25 years of age, pale, dark complexion, of a lymphatic sanguine temperament—of strong constitution, enjoying general good health, was imprudent enough, after inordinate exertions in the duties of his profession, to take, at a single draught, a large glass of iced water. During the night he was taken with a chill—called at six o'clock on the evening of the 3d to see him—found the following symptoms: anxiety, severe headache, eyes injected, white tongue, mouth open, intense thirst, nauseau, sleeplessness, urine scant with sediment, burning dry skin, full, frequent pulse, (115) dyspepnœa, painful persistent cough, *tearing* expectoration of viscid mucous, mixed with air and red blood—severe pain in the right lung, chiefly under the nipple—dullness over the inferior two-thirds of the lung of that side, with loud pneumonic crepitation. In spite of very low diet, appropriate drinks, rest, four free bleedings from the arm, (all more or less cupped,) within the space of thirty-six hours from my first visit, in spite of several large doses of tartar-emetic and ipecacuanha, two leechings, one to the painful part, the other around the inside of the ancles; in spite of blisters on the breast, and on the legs, synopism, emollient elysters, etc., the disease continued to progress.

At 10 o'clock, on the evening of the 9th, the local and general symptoms had continued to increase in number and gravity, until it was obvious that red hepatization of the lungs had taken place; face pale and anxious; extreme debility; lying all the time on the back; delirium—subsultus tendinum, vague expression, stupor, dry tongue; insatiable thirst for cold water; dry skin; eyes partly open; the eyeballs turned up; the pupils dilated; the cetina slightly sensible to strong light; passing urine involuntarily; pulse low, interrupted and diaphragmatic; respiration hard; cough scant and difficult expectoration, sputa thick and discolored; dullness increased and extended; respiratory murmnr, well over the seat of the disease; *rales* and other bronchial sounds; cold extremities; tendency to sinking towards the food of the bed.

Under the above circumstances, with no reasonable hope of evading an unhappy termination by ordinary means, we prescribed 15 centigrammes of the augueous extract of Belladonna, mixed ten grammes of "sirop de cappillaire," taken at one time.

At the end of an hour and a half the effect of the Belladonna was obvious, the patient slept tranquilly and quietly. In a few minutes his temperature augmented, face became adimated, warm, vapory, and anundant perspiration covered the body; the pulse rose, became stronger and less frequent; respiration was relieved; the symptoms all became more favorable. After two consecutive hours of repose, the patient awoke free from delirium, and feeling much better.

At 10 o'clock in the morning the pneumonia, which had to a certain degree re-assumed the characteristics of the first stage, was in a fair way to terminate by resolution.

Convalescence continued up to the 16 of November, when it was complete.—*De La Rue.*

EDITORIAL.

History of the Epidemic Yellow Fever at New Orleans, La., in 1853. By E. D. FENNER, M.D., one of the Visiting Physicians to the New Orleans Charity Hospital; member of American Med. Association, &c. &c. New York: Hall, Clayton & Co, Printers, 46 Pine St., 1854.

THIS is a Monograph of 84 pages; embracing a pretty full historical, statistical, sanitary, and medical record of that exceedingly fatal epidemic of Yellow Fever which scourged New Orleans and other towns on the Gulf coast and lower Mississippi so severely during the summer and autumn of 1853. The value of the paper is also enhanced by the addition of a few pages containing the conclusions arrived at in the "Second Report of the General Board of Health of England on *Quarantine* in Yellow Fever."—

Dr. Fenner in the work before us gives a very full and exceedingly interesting account of the rise, progress, and termination of the epidemic in the city of New Orleans. He gives an equally frank and full statement of the sanitary condition of the city at the time, showing the local causes of disease to have been most shamefully abundant. He also gives the meteorological and climatic features of the season. But we have only time now to state his conclusions in reference to those points which are of great interest, not only to the profession, but equally so to the municipal authorities of all cities and towns. The Report of the General Board of Health of England was signed by the Earl of Shaftsbury, Mr. Edwin Chadwick, and Dr. T. Southwood Smith. Dr. Fenner remarks: "I must say that my humble observations fully confirm the remarks and conclusions of this learned and able commission." Their conclusions he gives as follows, viz.:—

"From a consideration of the whole of the preceding evidence respecting Yellow Fever, we have arrived at the following conclusions:—

"1. That Yellow Fever Epidemics break out simultaneously in

different and distant towns, and in different and distant parts of the same town, often under circumstances in which communication with infected persons is impossible.

"2. That Yellow Fever Epidemics are usually preceded by the occurrence of individual or sporadic cases of the disease, which sporadic cases are likewise common in seasons when no Epidemic prevails.

"3. The Yellow Fever Epidemics, though occasionally extending over large tracts of country, are more frequently limited as to the space over which they spread, often not involving the whole of a town, and sometimes not even any considerable district of it.

"4. That Yellow Fever Epidemics do not spread from district to district by any rule of gradual progression, but often ravage certain localities, while they spare entirely, or visit very lightly, others in the immediate neighborhood, with which the inhabitants are in constant intercommunication.

"5. That Yellow Fever Epidemics, when they invade a district, do not spread from the houses first infected to the next, and thence to the adjoining, and thus extend as from a centre; but, on the contrary, are often confined to particular houses in a street, to to particular houses on one side of a street, to particular rooms in the same house, and often even to particular rooms on the same story.

"6. That in general, when Yellow Fever breaks out in a family, only one or two individuals are attacked; commonly the attendants on the sick escape; and when several members of a family are successively attacked, or the attendants on the sick suffer, either the epidemic was general in the locality, or the individuals attacked had gone into an infected district

"7. That when Yellow Fever is prevalent in a locality, the most rigid seclusion in that locality affords no protection from the disease.

"8. That, on the other hand, so great is the success attending the removal from an infected locality, and the dispersion of the sick in a healthy district, that by this measure alone the further progress of an epidemic is often arrested at once.

"8. That such dispersion of the sick is followed by no transmission of the disease, not even when the sick are pl ced in the wards of a hospital among patients laboring under other maladies.

"10. That no one of the preceding facts can be reconciled with any other conclusion than that, whatever may be the exciting cause of Yellow Fever, it is local or endemic in its origin: and the evidence of this conclusion is, therefore, cumulative.

"11. That the conditions which influence the localization of Yellow Fever are known, definite, and, to a great exten remov-

able; and are substantially the same as the localizing causes of Cholera, and of all other Epidemic diseases.

"12. That, as in the case of all other Epidemic diseases, in proportion as these localizing causes are removed or diminished, Yellow Fever ceases to appear, or recurs at more distant intervals, and in milder forms.

"13. That besides the common external localizing causes, there is one constitutional predisposing cause of paramount importance, namely, non-acclimatization—that is, the state of the system produced by residence in a cold climate; in other words, European blood exposed to the action of tropical heat; the practical lesson being, that the utmost care should be taken to prevent individuals or bodies of men recently arrived within the Yellow Fever zone, from going into a district in which the disease actually exists, or has recently been present.

"14. That there is no evidence to prove that Yellow Fever has ever been imported.

"15 That consequently the means of protection from Yellow Fever are not quarantine restriction and sanitary cordons, but sanitary works and operations, having for their object the removal and prevention of the several localizing conditions, and when such permanent works are impracticable, the temporary removal, as far as may be possible, of the population from the infected localities.

"We deem it our duty to state, in conclusion, that from the most careful examination which we have been able to make of the mass of evidence submitted to us from which the foregoing conclusions have been deduced, we have not found a single fact or observation clearly ascertained and authentically recorded, opposed to the general tenor of such evidence. We have met with no exceptional cases. We have indeed found the opinions of some authorities, for whom we entertain great respect, not in accordance on some points; but these have reference for the most part to matters of a purely professional and scientific nature. On the great practical question, whether, whatever may be the nature and mode of propagation of Yellow Fever, Quarantine and Sanitary Cordons can afford any real protection against its introduction and spread, we believe there is now a very general unanimity of opinion, in accordance with the evidence we have submitted, that they cannot. We believe there is the like general agreement in this further practical conclusion, that the substitution of Sanitary or Hygienic measures for Quarantine isolation and restriction, would afford more certain and effectual protection."

A Text Book of Anatomy, and guide in dissections for the use of Students of Medicine and Dental Surgery. By Washington R. Handy, M. D., Prof. of Anatomy and Physiology in the Baltimore College of Dental Surgery, etc.

This is an elegant work, containing 785 pages, with 264 wood-cut illustrations.

While this branch of Science, in its present state of perfection and completeness, offers but slight opportunity for original contribution, yet we have long entertained the opinion that much might be done, not only in the way of an improved arrangement of the subject, but more especially by rendering its dry and rugged details, in language more attractive; and we believe this object has been aimed at, if not fully attained in the work before us; the style being easy and familiar, while the minutest details are set forth clearly and definitely.

Although this work is professedly designed to meet the wants of both Medical and Dental Students, yet the demand particularly originates with the latter, who have long expressed the want of a production more adapted to their profession than the ordinary works on Anatomy; and in meeting this want the author has given special attention to the anatomy of the mouth, together with its anatomical and physiological relations to the general system.

The author commences with a lengthy introduction, comprising a history of the science of anatomy, and a general outline of organization, while the body of the work is divided into four parts.

The first treats of the elementary tissues; their anatomy, origin, physical and vital properties, etc.

The second part treats of the head; combining in the description of the various organs, both their anatomy and physiology, together with their functional and anatomical relations.

The third part describes the trunk, in the same physiological order; ending by showing the relations existing between the mouth and its several organs and viscera.

The fourth part treats of the extremities in the ordinary manner.

Altogether we believe this work to be, in many respects, a decided improvement, and feel safe in recommending it to the professional public, not only for its accuracy, but more especially the at-

tractive style, in which the author treats his subject, a great desideratum with a large proportion of Students, who are apt to come to the conclusion that "Anatomy is a dry study." F.

Letter from La.

[The following extract from a private letter from our much respected friend Dr. Huey, will interest many of our readers.]

ATCHAFALAY PARISH, ST. LANDRY, LA.
Jan. 25, 1854.

DR. HERRICK:—The diseases of this section are such generally as will be found most commonly throughout the great valley of the Mississippi. Itermittents, Remittents, Typhoid, Pneumonia, Rheumatism, Dysentery, Diarrhœa in a common way. This summer has formed an exception, in the way of Epidemic Yellow Fever, a few cases of which was encountered in my neighborhood, at least it was the same disease which prevailed throughout the South this summer and fall, and was most generally pronounced Yellow Fever, though it was denied as being Yellow Fever by some of the most eminent Physicians of New Orleans, and other localities; for example Dr. Warren Stone, of New Orleans, said it was a combination of Yellow Fever and African Plague. I have not seen anything from his pen regarding it, but that is the general report respecting his opinion concerning it. The symptoms of the fever which prevailed here, and which I attended and treated (some seven or eight in number) were—first, an indisposition, slight for a longer or shorter time, generally 12 or 24 hours—when would ensue severe pain in the head and back, more so than is generally found in remittent fever, great suffusion of eyes, which were dull and heavy looking, tongue red and broad with a white mucus in the middle in some cases—and in others bilious coating in the middle. Bowels costive, great heat of surface generally; nausea and vomiting—this was *most generally* indeed a troublesome symptom, and the character of the matter vomited was bilious. Pulse—from 110 to 120 in the minute, generally full, these symptoms were high for 3 or 4 days, when they would assume more of the typhoid type. Another prominent symptom was bleeding from the nose and mouth, and in one case, continued oozing of blood from the gums lasting 9 days.

The character of the discharges from the bowels, showed [illegible] formed its principal ingredient. The treatment was at first to use the warm mustard bath (that is foot-bath); when they would be wiped dry, placed in bed, and given from 10 to 20 grains blue pils or, Cal. gr. 10 Dov. gr 8 and at the expiration of 8 hours, a large dose of Caster Oil and Spirits Turpentine, to insure free action on Bowels. For sick stomach. mustard plasters were applied, in connection with soda powders given internally; and after the bowels were freed and the sick stomach measurably ceased, I gave a diaphoretic of Nitre and Dig., most generally. In one case, that of a stout young man, after the operation of the cathartic as above, I commenced with the Quinine in large doses, (on the evening of the second day of the fever) 60 grains in 24 hours, at intervals of 8 hours 20 grains, till he he had taken in the 48 hours 120 grains, which was followed by profuse diaphoresis, cessation of fever, and convalescence on the 5th day. In the other cases I did not use the Quinine to the same extent—but relied more on the diaphoretic proper—using an *expectant* treatment. In the case of bleeding as mentioned, in conjunction with the remedy mentioned, used the chlorate of potash 40 grains to 3 oz. water, in table spoon doses once in 12 hours. The potash was used to supply oxygen the blood, which was considered deficient in that particular. In this case there was in connection with the symptoms stated *great* lessness during the whole time, at least till convalescence ensued.

This is a rough sketch of the disease, as was seen here, and I suppose you would like to hear, whether they got well or died.—With pleasure I can say they all recovered. Now, whether it was owing, in any great degree, to my treatment or not, I cannot tell as Prof. Fitch of R. Med. College used to tell us, our patients may get well, in *spite* of our treatment, and I confess this has often occurred to my mind, and has perplexed me much.

I have a case to relate to you, of a different character. A young lady now 18 years of age, in the year 1850, was taken with Parotitis on both sides which continued in the usual way, for 5 or 6 days, when she began to get better. At this juncture she imprudently exposed herself in a damp gallery, and was taken with

fever the swelling returned, and extended to the clavicles and even behind the ears—in fine the whole of the throat and jaws swelled prodigiously, and interrupted the breathing very much—and in connection with this and the fever, vomiting came on one evening during the efforts of which she became partially deaf, and the next day, she became totally deaf, and has remained deaf ever since. The fever continued for some time afterwards, and she had in common terms, what is called a long spell of sickness. She was taken in the summer and did not *go about* for 8 months. Ever since this spell, her health, contrary to all expectations, has been tolerably good—and she retains to a remarkable degree, the power of speech, and that with good modulation and intonation of voice. One unacquainted with her, on hearing her talk, would hardly suppose her to be deficient in hearing. She is lively in disposition, and intelligent. The glands around the throat are ar dened, and the opening of the Eustachean tube within the mouth seems to present some degree of contraction. The tube is not entirely closed, at least was not a year ago. There is little or no secretion from the external ear, and there has never been any running of any kind, indicating anything like disorganization of the parts; but the ringing of the largest bell, over her head, she cannot hear. The firing of a cannon, or report of a gun, makes an impression through the sense of touch. Now the small bones of the ear may be affected, Paralysis of the auditory nerve in connection with the partial occlusion of the Eustachean tube may suffice to produce the result, I suppose, but to see the cause certainly is an object of inquiry. She is frequently troubled now with a roaring or rumbling in her ears—and I tried the effect of Quinine on her, and it produces a ringing in her ears—as she expresses it.

Your Friend,

GEORGE J. HUEY.

Iodine an Antidote to the Poison of the Curare.

We find in a late number of the Gazette des Hopitaux the following which we are sure will be interesting to our readers.

Messrs Brainard and Green have communicated to the Academy (of Sciences,) a paper having the following title, On the use of Iodine an antidote to the poison of the Curare. One of the authors (M. Brainard,) has previously published the results of his experiments concerning the action of Iodine as an antidote to the bite of certain Crotali. In the new paper the experiments detailed go to show that the same solutions of Iodine have a simi'ar influence over the American poison known as the Wooraro or Curare.

We have remarked, say our authors, a great analogy between the action of these poisonous preparations and the effect of the bites of of some of the American Serpents, In adition it has been well established that the venom of these serpents has at least something in common with these poisons; we have been led to experiment with the same remedies as antidotes, which we have previously found as efficacious against the bite of the serpents.

As they have found preparations of the Curare which differ in strength, the two authors have in the first place determined the activity of that which they made use of in their experiments and which had been given to them by M. the Prince Ch. Boneparte.

One Centigramme was sufficient to render a pigeon incapable of sustaining itself in 8 or 10 minutes, although it continued to live for an hour or more in a kind of lethargy, it was difficult to determine the exact moment of its death. It is neccessary to use but a very small quantity of the poison, otherwise its effects are so rapid that there is not time to have recourse to the antidote.

We have found that 2½ centigrammes are sufficient to kill a Guinea Pig in three minutes.

We mingled in a glass mortar five centigrammes of the poison triturating it with 20 drops of distilled water.

Experiment first.—We injected under the skin of a Guinea Pig, 10 drops of this solution, the operation was scarcely terminated before the animal fell on its side, and in about three minutes it was completely dead.

Second Experiment—We mingled 10 drops of this solution with 30 drops of an Iodine solution prepared as follows:

Iodine - - - -	0,50
Iodid potass. - - -	1,50
Aquæ dist. - - - -	24,00

This mixture was kept for 30 minutes at the temperature of the body, and then injected under the skin of a Guinea Pig. the operation did not appear to effect the animal, the next morning it was apparently well.

Third Experiment.—As it might be objected that in our second experiment, the dilution of the poison in the water diminihsed its activity, we mixed 10 drops of the solution of Curare with 30 drops of distilled water, the whole was then injected under the skin of a Guinea Pig, it died at the end of three minutes.

Fourth, Fifth, and Sixth Experiment.—We mingled 12 drops of the solution of Cerare with 60 of the solution of Iodine; this mixture kept at the temperature of the body for 20 minutes, was divided into three equal parts, and injected under the skin of three pigeons, none of which were affected.

Seventh Experiment.—We mixed 4 drops of the solution of curare with 20 drops of the solution of Iodine, and injected the mixture immediately under the the skin of a pigeon which was not affected in the least.

As it might be supposed that the dilution of the poison in the water diminished its activity, we have assured ourselves that it is not so. This fact has been placed beyond doubt, by the results of the eighth, ninth and tenth experiments.

Eleventh and Twelfth experiment.—In order to place the subjects for experiment in the condition of those poisoned by a bite, we took two pigeons and injected under the skin of each 4 drops of the solution of Cerare, Immediately after we injected 20 drops of the Iodine solution in the wounds of each of the two pigeons, applying cups which were kept on five minutes. The two birds did not appear to suffer from this experiment.

Thirteenth Experiment.—After having shaved the flank of a Guinea pig. we injected under the skin 2½ centigrammes of the curare dissolved in 60 drops of water. Then we injected immediately the same quantity of the solution of Iodine; we applied cups, and the animal was not affected.

Fourteenth Experiment.—We injected 2½ centigrammes of the curare with 30 drops of water under the skin of a guinea pig, and submitted it to no treatment; it died in five minutes.

Fifteenth Experiment.—We injected under the skin of a pigeon 4 drops of the solution of curare, 30 drops of the solution of Iodine, and 30 drops of water; no effect was manifest.

Sixteenth Experiment.—We injected 4 drops of the solution of curare and 10 drops of water; the animal died in one hour.

Seventeenth Experiment.—We injected 5 drops of the solution of curare with 30 drops of the solution of Iodine and 30 drops of distilled water without any unpleasant effect.

Eighteenth Experiment.—In this experiment and the following we used a poison brought from the borders of the Amazon and known as the *ticunas*. This substance has the brilliant appearance of resin and a strong odor. It is known that it has been prepared less than four years. We used a solution of 5 centigrammes of the ticunas to 20 drops of water; 5 drops of this solution injected under the skin of a pigeon produced death in seven minutes.

Nineteenth Experiment.—We injected 8 drops of this solution of ticunas and two minutes after 20 drops of the Iodine solution and 20 drops of distilled water; we applied cups for 3 minutes. No effect was manifested.

Twentieth Experiment. We injected 8 drops of the solution of ticunas; then 20 drops of the Iodine solution; no effect was produced.

We believe ourselves justified in drawing from the preceding facts the following conclusions:

1st. The solution of Iodine and Iodide of Potassium is within certain limits a perfect antidote to the curare; mixed with it (as in solution) it destroys its poisonous effect.

2d. The Iodine solution injected immediately after the solution of curare neutralises completely its effects, provided that care be taken to apply a cup in order to arrest the circulation, and thus allow the Iodine to come in contact with the poison. It produces neither suppuration nor loss of substance by gangrene.

3d. The Iodine solution applied upon the surface of a deep wound of the muscles, in which the curare has been introduced, prevents the effects of the poison.

4th. The solution of Iodine has upon the curare an action perfectly identical with that which we have previously shown it to possess over the poison of the crotalus (Mem. presented to the academy Nov. 28th, 1843.)

5th. The identity of the effects of the curare and the poison of the crotalus, their same odor, and the effect of Iodine in controlling their actions, give much weight to the opinion already sufficiently indicated, that the active principle of the curare and analogous preparations is identical with that of the crotalus preserved in a particular manner.

Iodine—an Antidote to the poison of the Rattlesnake.

Messrs Eds:—I see from the February No of your Journal and also from the newspapers, that Prof. Brainard of Rush Medical College (now in Paris) claims to be the first discoverer, that Iodine is an antidote to the poison of the Rattlesnake. The fact may be, and probably is original with him. But I deny that he was the first to discover it, unless he made the discovery previous to the 14th day of September, 1848, for it was at this time that I made the first application of Iodine to the human subject, for the purpose of neutralizing the poison of the Rattlesnake, and with entire success, on a boy about 15 years of age, residing in this neighborhood. I saw him him about six hours after he was bitten and found the limb (leg) somewhat œdematus; made external application of Tinct. Iodine, with camel hair pencil, and gave 20 drops internally once in four hours until swelling subsided. After a few doses and applications the boy recovered without any other treatment.

I have since used it soon after the wound was made, externally and alone. with the same result, and I am now perfectly satisfied that the Iodine treatment, thoroughly carried out, will deprive the Snake of all his terrors.

I do not deem it necessary to inject the Iodine into the tissues

in order to obtain its beneficial results. Because the alcoholic tincture which I use will be as readily and more perfectly absorbed than the poison itself. And I know the tinct. to be much better than the Iodid. of Potass. or Aqueous solution, for the reason that it is more readily absorbed by applying to the surface of the body than any other preparation of the Iodine.

Yours, &c.

JOLIET, April 10, 1854. E. HARWOOD, M. D.

[In Vol. 1st of the North-Western Medical and Surgical Journal, page 396, is an article by Dr. Whitmire of Metamora, Illinois, in which he reports a case in his practice in June, 1846, treated by the external application of Tinct Iodine. If there be any honor in priority of use, it is due to Dr. Whitmire.

EDS. JOURNAL.]

Cook County Medical Society.

THE regular monthly meeting of this Society was held in the office of Dr. N. S. DAVIS, on the 4th of April. The President and Secretary both being absent, the meeting was called to order by the Vice President, and Dr. H. Smith appointed Secretary pro tem. The communication of facts and cases being called for, Dr. D. A. Colton alluded to some cases of remittent fever, and made the inquiry whether, when sulphate of quinine given in two or three grain doses produced unpleasant symptoms, such as ringing in the ears, nervousness, &c., the *dose* should be *increased* with a view to induce the sedative effect of the remedy; or whether it should be modified by the addition of other remedies or superceded altogether?

Dr Schneider related some interesting facts in relation to the use of quinine in one of the military hospitals in Europe, from which he inferred that quinine was very often borne with less cerebral disturbance if given immediately after the operation of a mild cathartic, or in combination with a few grains of pulv. rhei. Similar facts had also been noticed by other members of the Society.

Dr. N. S. Davis related some cases of erysipelas; one particu-

larly of the face and scalp, in which the muriated tinct. of iron was given in doses of 20 drops every four hours, with prompt and decided benefit. The tinct. of iron was not exhibited until the third or fourth day; and had been preceded by alterative doses of calomel combined with opium.

On inquiry among the members, it was found that erysipelas had been more prevalent than usual in the city, during the last two weeks. Two or three deaths had been reported from that disease; and Dr. Davis said he had met with six cases recently in his own practice, and all but one in the southern part of the city.

The reading of essays having been called for, Dr. F. Schneider, read a paper headed, "A new method of extracting the retained placenta." The paper of Dr. Schneider referred chiefly to those cases of retention of the placenta requiring the use of the proper forceps for its extraction; and the novelty proposed was in the mode of making the *tractive* force. He said the chief immediate dangers encountered in removing adherent and retained placentas, were hœmorrhage, and inversion of the fundus of the womb. The Doctor related some cases which had led him to the conclusion that if, when the retained body was seized by the forceps, the traction was preceded and accompanied by a twisting or rotary motion of the instrument, the adherent parts would be detached with less liability to hœmorrhage or inversion, while the same manipulation would strengthen the placenta itself, and render it less liable to be torn so as to leave a part behind. Dr. Schneider's paper was an interesting one and elicted some discussion.

He was also requested to place a copy on file among the papers of the Society.

The present meeting was the proper one for the annual election of officers; but several of the older members of the society being absent, that business was postponed.

And on account of the meeting of the American Medical Association on the first Tuesday in May, it was resolved to hold the next monthly meeting on the *second Tuesday* of May next; after which the Society adjourned.

HIRAM SMITH, M. D.,
Secretary pro tem.

Aurora Medical Association.

At a meeting called for the purpose of forming a Medical Society, pursuant to an adjournment of a preliminary meeting held at Aurora, March 6th, Dr. O. A. Lovejoy was called to the chair and Dr. A. Hard was appointed Secretary.

The preliminary steps were then taken for the organization of a Society, to be known as the Aurora Medical Association, and Drs. Angell, Young, and Hard, were appointed a Committee to prepare a Constitution and By-laws, for its permanent organization.

On motion, the Meeting adjourned to meet at 7 P. M.

Meeting called to order by the President. Dr. Angell, Chairman of the the Committee, reported that they had performed the duty assigned them; which report was accepted, and the Constitution and By-laws were, with some amendments, adopted.

The following permanent officers, were then elected for the current year, viz: Dr J. L Brooks, President; Drs. L. H. Angell, and J. E. Carr, Vice-Presidents; Dr. O. A. Lovejoy, Secretary; P. A. Allaire, Corresponding Secretary; Dr. D. W. Young, Treasurer; Drs. D. W. Young, J. E. Carr, and A. Hard, Censors.

On motion, the Secretary was requested to publish in such Journals as he thought proper, an abstract of the proceedings of this meeting. When,

On motion, the Association adjourned to meet again the 2nd Monday in April. J. L. BROOKS, Pres't.

O. A. LOVEJOY, Secretary.

A regular meeting of the Association was held at Aurora on the 10th inst.

Dr. D. W. Young, was appointed delegate to the American Medical Society. Dr. J. E. Carr, Substitute.

Dr. P. A. Allaire was appointed delegate to the State Medical Society. Dr. A. Hard, Substitute.

Dr. Ira Dales presented his credentials, and was admitted as a member of the Association. Drs. C. Wheeler and D. Eastman were admitted as Honorary Members.

Various matters of professional interest, were talked of and dis-

cussed, after which the association adjourned to meet again on the first Monday in May. O. A. LOVEJOY, Sec'y.

Aurora, April 13th, 1854.

Obscure Tumour—Sudden Death.

(From a private letter we make the following extract, the case is a singular one, a more complete history would be desirable. EDS. *Journal.*)

An interesting case came partialy under my observation during the past winter, would have been reported to you had any one, or any single set of men had the entire charge of it.

A child three years of age, early last summer had what is called a "stiff neck," with pain in the spinal column, at times application was made to me, and a simple treatment was adopted, by which the pain was relieved and the stiffness obviated. In the middle of the winter I received a summons to see the child and be prepared to remove a tonsil. Before I had made any examination whatever, I learned that they had been to a professed and competent surgeon, who had partially removed the tumor, and that it had grown more rapidly since the removal; they had again applied to him, and he had refused to operate further but gave them medicines, with directions which they had not followed. I then, without examination, refused to do anything for the child, and referred them to the first surgeon. After much entreaty on their part, I examined the case slightly, saw a tumor springing from the fauces, filling the entrance to the throat, and impeding respiration, it was hard to the feel. I did not determine its connections, again I refered them to the first surgeon, promissing to assist him if he thought best to operate and desired. I gave them no opinion whether I thought an operation best. But they still insisted that I should do so, when I told them I would do nothing without further council. This was procured, and the only determination was to return it to the first surgeon; which we finally persuaded them to do. The first surgeon would not operate, but they found two physicians who finally consented to try, all having expressed the opinion that the operation would be dangerous if not fatal. A cork was placed between the teeth, and as yet no instrument had touched the child, it faint-

ed and died. I regret that the tumor with its connections was not removed. I have been thus particular in the historical paart of the transaction, to show that a correct report could not easily be made. I was not present at the operation.

Query. what was the tumor? In this doctors disagree. Let us examine the circumstances. The tumor filled the fauces and partially the mouth, so completely that owing to the difficulty of breathing, it was scarcely possible to determine its connections, whether fauces tonsil or tongue. There appeared no enlargement of the opposite side, or redness of the surrounding parts. The submaxillary glands on the affected side were enlarged but not on the other. After the operation there was no hemorrhege, the operating surgeon after the death of the child, found the connections clearly joined to the tongqe with a wide short base or neck. The tumor was very brittle, holding a tenaculum but tearing out with the least force, internally it was of a fleshy character, rather colored, but hardly sarcomatous. Some regarded it as malignant and others as a polypus.* Those who examined it the most carefully, regard it as the latter. The tumor had not been fully removed at the time of the operation, and grew very rapidly afterwards.—Would it have been advisable to have performed tracheotomy before commencing the operation.

Truly Yours,

STORRS HALL.

[We learn from our correspondent that a medical society has been formed by the physicians of the county of Fond du Lac.—Dr. Wm H. Walker is the delegate to the American Medical Association. EDS. JOURNAL.]

The friends of Prof. Brainard will be gratified to know that he is reaping new honors in the old world. We present our readers in this number, an interesting letter from Prof. B., also a translation from the Gazette des Hopiteaux of his experiments on vegtable poisons. He is expected home about the 20th inst., J.

*In Chelius surgery is given a description of polyps of almost a similar character, but situated lower in the throat than this.